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Interactive comment on “Microphysical, macrophysical and radiative signatures of volcanic aerosols in trade wind cumulus observed by the A-Train” by T. Yuan et al.

Anonymous Referee #2

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A volcano located on the Hawaii Islands injected massive load of sulfur to the atmosphere for a few months during 2008. This provides an opportunity to analyze the impact of CCN on the cloud life cycles. This paper analyzes this particular natural experiment through the use of various spaceborne observations of atmospheric parameters over the Pacific. They demonstrate that, as hypothesized before, aerosols have a large impact on the aerosol life cycle, with a decrease of the droplet radius, a suppression of precipitation, an increase of cloud fraction. Overall, the aerosol load has a large impact on the albedo, which a dominant contribution by the indirect effect. This paper is certainly very interesting and convincing. As stated by the authors, there have been several demonstration of the aerosol impact on cloud microphysics,

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but this one goes a step further as it demonstrates the impact on the cloud fraction and Earth Albedo. There is no doubt that the paper is a very significant contribution to the study of aerosol-cloud interaction, and it should therefore be published. I have very few suggestions for correction as the paper is very well written and presented.

I must admit I have had difficulties following the methods used to discard the Island wake effect impact on clouds, to finally discard it. I was not convinced by the method that uses EOF for various meteorological fields. It seems clear that the wake Island effect is similar from year to year. Is the SST pattern significantly different in JJA 2008 than for the same months of other years? Note that, even if a lower SST is observed in 2008, one may argue that it is a consequence, rather than the cause, of the cloud cover change. I did not feel that the EOF analysis is necessary to discard the wake effect as the cause for cloud parameter changes downwind of the Island. I therefore suggest that the authors reconsider whether such analysis is really relevant.

The most important comment concerns some of the Figures (3,4,6). I assume the input data are 1 degree resolution monthly means. There is obviously some spatial interpolation/extrapolation on these data that are not needed, and not desired as one does not know its impact. I strongly suggest the authors keep with the original resolution with no spatial interpolation.

Figure 1A and b should have the same spatial area. If possible, all figures should have the same area and projection.

Figure 12 shows the relative humidity anomaly as given by a re-analysis. However, it is not clear which data have been used to constrain this analysis. It is neither not clear whether the patterns are significant. I do not think this figures add significantly to the demonstration

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